THURSDAY, AUGUST 18, 1892.

A DEBATABLE LAND—PLANTS OR ANIMALS?

A Monograph of the Myxogastres. By George Massee. (London: Methuen and Co., 1892.)

THIS work is much in advance of any book in the English language treating of the perplexing group of organisms that forms its subject. The author has been in peculiarly favourable circumstances for the preparation of such a monograph, having enjoyed full access to "the splendid collection of Myxogastres in the Royal Herbarium, Kew, rich in types, and with numerous annotations by Rostafinski," as well as had "the loan or gift of valuable, and in some instances unique, specimens" from other workers in the same field in different countries. He has fully acquainted himself with the literature of the subject, and has made many personal observations on the structure, and, in some cases, on the life-history, of various types. He is thus able to bring to bear on the discussion of the problems that claim consideration a wide and varied knowledge; with the result that the book is indispensable to every student of the Myxogastres. The introductory portion will be found worth perusal by others besides specialists, as it discusses the arguments for and against the vegetable nature of the group. While accepting the view that the origin of the Myxogastres is to be found among the *Flagellatæ*, he comes to the conclusion that the sum of the characters presented by them in the reproductive phase manifests a tendency "in the direction of the vegetable kingdom, and more especially in the direction of the Fungi." But he is unable to establish strict homologies with the latter; since the Myxogastres are "a terminal group, and permit no comparisons with higher forms of the same type." In the discussion of this vexed question, Mr. Massee shows none of the virulence to which it has given rise in former times; and he endeavours to do justice in his statement to the views of De Bary, and of other supporters of the view that the organisms in question should be regarded as animals. After all, to an evolutionist at least, the distinction would appear more of verbal than of real importance.

Accepting the view that there are certain forms, of a very primitive structure, from which the animal and vegetable kingdoms have been developed in ever-increasing specialization, there is cause to expect the existence of more or less intermediate types, in which the characters at one period of their life-history are more those of animals, and at another period more plant-like. It matters little under which kingdom we agree to place them. Here, as elsewhere, Nature refuses to be bound down by rigid classification, and we must accept facts as they are, not as any *a priori* system might wish to make them. That the Myxogastres are not Fungi may be admitted; though they show a considerable similarity in various points in the course of development—a similarity clearly stated by Mr. Massee in his discussion of the whole question.

The term *Myxogastres* is employed, we infer, on the ground of priority. It possesses, however, the incidental advantage of not implying any positive view on the nature of the group, in the same way as do the terms *Myxomycetes* and *Mycetozoa*. The limits of the group are taken in this monograph in the sense employed by Rostafinski. It thus does not include the *Acrasieæ* and *Ceratium*, admitted as Mycetozoa by De Bary; nor does it make any reference to the numerous forms of the *Monadineæ*, which Zopf discusses in his "Schleimpilze" in Schenck's "Handbuch der Botanik." Thus limited, the group is more homogeneous; though the definition is, perhaps, somewhat arbitrary, and omits forms that are undoubtedly related to the more specialized types, and an English work on which would be welcome.

The author enters on somewhat slippery ground in the endeavour to explain the line of development of the Myxogastres, and also to illustrate his ideas of the relationship between the several orders. He holds that four orders can be distinguished by the presence or absence of lime in the sporangial wall, and by the presence and nature of the capillitium.

> "In each order we find the special characteristic idea evolving through a sequence of genera, the terminal one not connected with any higher order, hence the special feature terminates abruptly within the order where it originated, and it is invariably in some comparatively undifferentiated genus near the initial point of each order, that we meet with the suggestion of a new line of evolution, which, at its maximum of development, constitutes the characteristic feature of the order immediately in advance of the one from which it emanated in an incipient condition."

Turning now to the systematic portion of the work, we find that it gives abundant proofs of care and of familiarity with the several forms, based on personal examination of each. The method of description is clear, the more important characters being printed in italics. Mr. Massee recognizes fully the difficulties of determining the limits of species and of the larger groups "while the life-history of the majority of forms is still unknown," saying plainly that

> "all attempts at classification, as also the conception as to what constitutes a species, must be considered as tentative. When we are better acquainted with the main lines of development and lines of variation, also the conditions determining these variations, it is certain that the main factor in the discrimination of species will not be a one-twelfth oil-immersion objective."

Basing his acquaintance with the Myxogastres on personal examination of large numbers of examples, fresh and dried, many of the latter being authentic types, Mr. Massee does not hesitate to unite species and genera hitherto kept as distinct, but shown to be connected by fuller material. Thus several familiar names become sunk as synonyms; *e.g. Licea*, Schrad., and *Lindbladia*, Fr., are ranked under *Tubulina*, Pers. (emended). Under the generally accepted rules of nomenclature, this leads to Massee standing as the authority for many species, transferred by him, in reality, to another genus. But, besides such cases of apparent novelties, there are also a good many descriptions of *new* species in the usual sense of the term. The synonyms are carefully given under each group and species. A wise reticence has been observed in the endeavour to recognize the species meant by most of the older writers who mention the Myxogastres. The

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synonymy previous to Rostafinski's monograph is borrowed as a whole from that work, "without any attempt at corroboration." Mr. Massee says:—

> "I feel certain that nearly one-third of Rostafinski's work would not have been sacrificed to synonyms unless they mean something more than I have been able to discover, hence I have not felt justified in ignoring them altogether."

The geographical distribution has been worked out from the extensive collections already referred to as at the author's command.

The twelve plates, bearing 313 coloured figures by Mr. Massee himself, call for special mention as a valuable assistance to students of the Myxogastres. They deserve high praise for their accuracy and execution. The printing and get-up of the book are very satisfactory. A review would scarce be complete did it not find fault with some point or other; and we may do that part of our duty very briefly by taking exception to the rather inconvenient size (large octavo), and to the tendency in the introductory pages to let the sentences run to an inconvenient length. One, taken at random, we found to occupy twenty-five lines. There is no ground for this charge, however, as regards the descriptive portion of the monograph.

LETTERS TO THE EDITOR.

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The Apodidæ—a Reply.

PROF. LANKESTER'S review of my book in NATURE (p. 267) contains, as is usual with "candid opinions," a considerable number of misstatements. These compel me to ask space for a reply.

Prof. Lankester commences by stating very authoritatively that my account of the hermaphroditism of Apus is erroneous. This question, being purely a matter of evidence, can wait. My account of it in "The Apodidæ" is "meagre" because, as is clear to any one who reads the preface, I was constrained to put aside for the present all questions which did not directly bear upon the line of argument embodied in my book.

These points, however, are not serious. Let us turn, then, to the main charges which are intended to deprive my book of all claim to be a real contribution to zoological science. Prof. Lankester, after himself dethroning my title, "The Apodidæ," says that I "pose as the discoverer of a new and unsuspected agreement between the Crustacea and the Chætopoda, and that I bring forward arguments as new which have "long been effectively used" for the same purpose. It is difficult here not to accuse Prof. Lankester of deliberate misrepresentation. If he will allow me to keep my title and will read my book, he will find that I go beyond this general standpoint, and specialize the Apodidæ as the particular Phyllopods which are to be deduced from a Chætopod. Without, I believe, a single page of digression, my book discusses from beginning to end the relation of the Apodidæ to the Annelids, of the Apodidæ to Limulus, to the Trilobites, and so on. All the well-known arguments in favour of the more general proposition which deduces the Phyllopods from Annelids I have naturally adopted, adding, however, many new arguments of more or less weight in favour of my special point. Not one of these arguments does Prof. Lankester attempt to meet. The only one he refers to he wishes to claim as his own, as, indeed, he does everything else in the book "which will bear examination"! This charge of wholesale plagiarism from Prof. Lankester's articles on Apus and Limulus is the more remarkable, because my own investigations compelled me either to modify or to reject almost every position therein adopted by him. This may account for his "candid opinion," but hardly for his charge of plagiarism. The only evidence he adduces to support this charge can merely be meant to throw dust in the eyes; it is as follows:—

In describing the absence of articulations in the limbs of Apus I admitted that Prof. Lankester had noted the point (which, however, is not absolutely correct), but I added that he had failed to see its significance. Prof. Lankester resents this statement, and cites himself to show that he agreed with Claus in holding that the limbs of the Arthropoda were homologous with the parapodia of the Chætopods. This acquiescence in a general proposition does not in any way prove that he applied it to explain the special conditions of the limbs of Apus.

While I do not at all share his jealousy in matters of priority, and will gladly yield the point to him if he can base his claim on something more definite than the passage he cites, the fact that he wishes to claim this argument for his own is specially interesting. There is far more meaning in this than in his use of such expressions as "fanciful conceptions, crude speculations, and dogmatic assertions," because, if this particular argument holds—and Prof. Lankester would not claim it unless he acknowledged its validity—it goes far to show that my theory can hardly be called a "fanciful conception." The reviewer's statement that "there is no evidence" that I "made use of well-preserved material," looks as if he had not taken the trouble to read the book, and further as if he did not understand the importance of the issues at stake; the histological points, which are the only ones likely to be affected by the state of preservation of the material, are insignificant as compared with the main argument.

If, instead of indulging in such loose charges, Prof. Lankester had endeavoured to show where, in his opinion, my argument breaks down, and what are some of the more glaring misstatements in my book, which cause him to "regret" that he cannot recommend it as "a repository of fact," he would have done science (and perhaps (?) myself personally) much better service. I should also personally have been grateful to him had he himself set an example to the more "inexperienced" zoologist of "how morphological problems should be attacked." I did not, in my speculations as to the relation of Apus to the Annelids, feel inclined to follow the example set by Prof. Lankester in his own speculations as to the relations of Limulus to the Arachnids. I was especially recommended to ripen my ideas, and to publish them together in book-form. Would Prof. Lankester have advised me to publish my speculations, as he did his, in separate articles, occasionally, perhaps, advancing theories and arguments in one article which have to be withdrawn in the next? This plan may be convenient for the writer, but is most annoying to all who have to work over the same ground again.

To conclude, my book is an argument from beginning to end; the argument may be absurd, but it must be met by argument. In the meantime, until Prof. Lankester demolishes it, I have the good fortune to know that several leading zoologists, among whom Prof. Haeckel kindly permits me to mention his name, think it—well, to say the least—*not* absurd.

August 2. HENRY M. BERNARD.

Calculation of Trajectories of Elongated Projectiles.

(*Additional Note.*)

IT has been already pointed out (NATURE, March 1892, p. 474) that the range table of the 4-inch B.L. gun, selected by the authorities, afforded a more satisfactory test of the value of the coefficients of resistance than the results of the special experiments carried out with that gun in 1887. This range table was based on practice of 17/5/83, 7/3/84, and 21,23/4/84. The muzzle velocity was 1900f.s.; the weight of the shot 25lbs.; and the diameter of the shot 4in. But no information is given respecting the height of the barometer or thermometer. In this table the elevations are given at which the gun must be laid to obtain ranges of 100, 200, 300 . . . . 7600, 7700 yards, and also the time of flight for each range, expressed to the $\frac{1}{100}$th of a second for ranges below 5000 yards, and to the $\frac{1}{10}$th of a second for ranges 5000 to 7500 yards.

In calculating the ranges for elevations of 1°, 2°, 3° . . . . 20°, the temperature was supposed to be 62° F., and height of the barometer 30in., at the level of the gun. The coefficient κ was supposed to be 0·97 to adapt the tables to a head struck with a radius of two diameters.

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